these statistics have been prepared. How far these records are absolutely correct is beside the present question; all the data have been obtained with the same instruments, and are strictly comparable between themselves.

It has been the custom at the observatory to treat as storms or gales, all occasions in which the Robinson anemometer recorded the horizontal velocity of the atmosphere as equal to or exceeding fifty miles an hour. The total number of instances in the twenty-eight years under consideration is 321, or an average slightly below one a month. It will frequently happen in stormy periods that the critical velocity will be registered, and be followed by a partial lull in the storm, to be succeeded by gusts of greater force. If these periods of comparative calm have lasted for about twenty-four hours, the disturbance would count as two storms, although probably both are parts of the same atmospheric disturbance.

The greatest number recorded in a year is in 1868, when no less than twenty-eight gales were reported; and the least occurs in 1880, when there were only two, and these neither long in continuance nor great in violence. It is not without significance—though, of course, it is not intended to insist upon the coincidence—that when the number of storms is plotted as an ordinate with the time for abscissa, a rough curve can be drawn among them giving maxima at practically equal intervals of five years, from 1868, with minima at intermediate dates. Or, if we take the sums of the maximum years 1868, 1873, 1878, &c., it is found that in the six we have records of eighty three storms; while in the six years of minimum, 1871, 1875, 1880, &c., we have only thirty-seven.

It is scarcely necessary to refer to the direction in which storms approach the observatory, after what has been said of its geographical position. Roughly, they are all from the west, with slight deflections to the north and south. As a matter of fact, only five have deviated from this rule, and they have been either east or south-east, and have been comparatively slight in their character.

The time scale on which the velocities have been registered is not a very open one. The recording drum moves through rather less than one inch in an hour, and the habit has been to read and record the distance travelled from the commencement of one hour to the beginning of the next. The maximum hourly velocity is, therefore, not to be understood as the greatest in any sixty consecutive minutes, but as the greatest in one whole hour as marked by the clock. In this sense the following table, which exhibits the number of times the greatest hourly velocity has exceeded noticeable amounts, is to be understood:—

Recorded hourly velocity in miles.	No. of instances.
50–60	220
60–70	68
70–80	21
80–90	10
Exceeding 90	2

Here again, curiously enough, we find some slight evidence in favour of a five-year period. Not only do the two instances of the greatest velocity recorded in the observatory occur in years already noticed as those of maximum disturbance, but the average velocity of all storms in the years 1868, 1873, &c., is 59·2 miles, as compared with 57·9 miles in the years of minimum number.

Intimately connected with velocity, though probably a less accurate measure of the true force of a storm, is the pressure recorded per square foot. Here Bidston has long held a record for the British Isles, having placed to its credit a pressure of ninety pounds on March 9, 1871. The accuracy of this measure has often been questioned, and probably it is too great owing to the momentum in the moving parts of the machinery, but it is certainly the record of a far greater pressure than has ever been witnessed since. Considering over what small areas these excessive pressures are exercised, and the great variation that exists from moment to moment in the velocity of the wind when a storm is raging, it is not an impossible amount, but it would certainly be misleading to conclude that such a pressure was a measure of the force of the wind a few feet, or even a few inches, away from the pressure-plate. The hourly velocity on the occasion when this pressure was registered has frequently been exceeded, without reproducing similar pressures. Herein is represented the great difficulty in determining a simple relation between pressure and velocity, or, rather, the square of the velocity; for one may regard Hutton's law of wind-pressure on a given obstructing surface as satisfactorily proved. The fact of such accidents as that referred to destroys any value that can be drawn from averages, but as a mere matter of figures, it appears that the average maximum hourly velocity for all storms is 58·4 miles, and the mean of the maximum pressures 37·6 lbs. This would require the factor for multiplying the square of the velocity in miles, to obtain the pressure per square foot, to be greater than one-tenth, which is evidently and necessarily erroneous, since we are comparing the accidental momentary pressure with the average velocity obtained throughout the entire hour. Taking the extreme pressures for what they are worth, the numbers come out as follows:—

Pressure in lbs. on sq. ft.	Number of instances.
20–30	132
30–40	111
40–50	50
50–60	11
60–70	12

On two occasions the pressure registered was greater than 70 lbs. to the square foot, and on three did not reach twenty. Since the extreme pressures are in a sense accidental, and do not represent with any accuracy the force of a storm, it does not seem desirable to determine the relative pressures in what have been called years of maximum and minimum storm occurrence. But the general features are again borne out. The explanation here is probably that the greater the number of storms the greater the chance of finding a high pressure. It seems more profitable to inquire what is the average length of a storm, how long may a violent disturbance be expected to last. This question is unfortunately complicated by the fact already alluded to, that a storm may subside for a few hours and then reappear with its original violence. If the interruption is only for an hour or two, as already explained, the depression is considered as one, but only those hours are counted in which the registered velocity exceeds fifty miles. There are only two instances in which this amount of violence has been maintained for thirty consecutive hours, viz. in February, 1868, and again last February. The total number of stormy hours in the twenty-eight years is 1732, giving an average of 5·4 hours for each storm. Our local disturbances are therefore not of long duration.

An examination of the dates when gusty weather is most prevalent, goes neither to substantiate the ancient myth of the equinoctial gales, nor to uphold the evil supremacy which has long been assigned to the winds of March. Since no attempt has been made to equalise the lengths of the months, February has been somewhat unfairly treated in the following table; but notwithstanding this handicap, it possesses the unenviable privilege of compressing within its shortened limits more tempestuous weather than any other month.

Month.	No. of storms.	No. of stormy hours.
January	47	260
February	42	281
March	47	238
April	14	63
May	7	27
June	3	19
July	7	21
August	17	65
September	16	77
October	26	180
November	44	254
December	51	247
Total	321	1732

Of course one would expect to find the greatest number of storms in the winter; but that three-fourths of the whole should be compressed within five months of the year, is a greater disproportion than was expected. The variations of the barometer during these storms, and the dependence of this variation upon the direction of the wind, are of considerable interest, but may not very well be entered upon here while treating simply of numerical statistics. WILLIAM E. PLUMMER.

Peculiarities of Psychical Research.

MR. DIXON asks in his first letter: Could an abnormal distribution of the cards affect the result, if certain precautions were taken? In his second letter he says there was nothing in his first letter to indicate that he under-estimated the import-

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ance of "abnormal distributions." Well and good, if the S.P.R. have not under-estimated the importance of examining the actual distribution of cards cut and of cards guessed, they will have kept a record of each card cut and each card guessed, card for card. If they have not done so, then their experiment is scientifically of no value; if they have done so, then the analysis of the distributions of the cards cut and the cards guessed ought to have accompanied any publication of these experiments. It is an obvious, but by *no means sufficient*, condition for a proper experiment. If the Secretary of the S.P.R. will place in my hands the actual analyses of the cards cut and the cards guessed made by a competent mathematician, *before* the publication in their *Proceedings* of the card guesses, and proving that they did at that time fully consider the point, and take this obvious precaution against deception, my estimation of the "scientific acumen" of the S.P.R. will at any rate on this point be modified.

I, of course, do not refer to my friend Prof. Edgeworth's investigations, which do not touch the question of the distributions of cards cut and cards guessed. KARL PEARSON.

MAY I call attention to Prof. Lodge's method of "silencing" me in your issue of January 10. It bears very closely upon this question of the effect of psychical research upon the investigator's reasoning. He quotes the preface of Mr. Podmore's book to show that that gentleman is not a "bigoted upholder of the certainty of telepathy," and the casual reader would scarcely guess that, in truth, I never asserted that he was. I complained of the very air of open-mindedness in that preface to which Prof. Lodge's quotation witnesses, and showed by an instance, that in the body of the book question-begging occurred which was all the more dangerous on account of the liberal tone of the opening portion. I made no objection to the individual prosecution of psychical research—only to its public recognition before it has produced more definite results than it has done so far. So much for the "silencing." It shows either that Prof. Lodge has not read my review, or that he has misunderstood it; and in either case it enforces my contention that these investigators are over-hasty. The phrase "irresponsible detractor," points in the same direction. H. G. WELLS.

The Suspended Animation of Snakes.

IN NATURE of December 6, p. 128, Mr. G. E. Hadow asks whether the snakes feign death for protective purposes, with intent to deceive, or whether the strange action is the result of a general nervous inhibition, produced reflexly by the action of fright, which would render it more or less analogous to a fainting fit. He and others of your readers will be interested in an additional observation that, in a measure, answers his question. The snake, a "hognose," "spreading adder," or "blowing viper," *Heterodon platyrhinus*, upon which Dr. L. C. Jones based his note in NATURE, November 29, p. 107, the origin of the discussion, was presented to me about five months ago. While in my possession it has repeatedly verified Dr. Jones's statements; and, besides, it has proved that it does not depend upon the feint alone. The latter is preceded by another action that apparently has not been published hitherto. After being teased a little, the animal, vigorously bending from side to side, the tail abruptly raised and the vent slightly protruded, begins to smear itself over the back with urine and excrement, the odour of which is so excessively nauseous that observers are quickly driven back, the better satisfied if they escape without a spatter in their faces. If the teasing stops with this, the victim glides away to hide; but if still more worried, it takes up the contortions that end in the trance-like condition, lasting ten minutes to half an hour, or until the creature feels that it may safely revive. The specimen still lives, and does not discard its filthy habit on prolonged acquaintance Much handling and familiarity with annoyance make little difference in behaviour, or in disposition to take advantage of the peculiar tactics. In the inception of the habits these actions most likely were due to terror; possibly the trance was a real faint; but, however their utility may have been discovered, it is evident at the present time that confidence in them as means of securing immunity from torment induces their practice on occasions when the existence of actual fright is hardly possible. At such times it would be difficult to convince witnesses that the snake is not intelligently employing what it knows to be its best methods of protection.

Cambridge, Mass., Dec. 27, 1894. S. GARMAN.

I DO not think that Mr. Vincent can be right in supposing that the suspended animation of grass snakes has nothing to do with simulation. I have never observed it in the case of a snake when unmolested in a glass case (as he has), though I have kept hundreds so, but noticed it first when catching snakes in the New Forest. After much struggling and the usual offensive methods of defence had proved vain, one has, in several instances, suddenly hung limp and apparently lifeless on my hand. It could hardly be a faint or anything but death-feigning, for as soon as I put it on the ground, or allowed it no longer to feel my hand, it recovered at once, and was off like a shot. I took particular pains to test this, as I was much surprised at the circumstance, which I did not remember having seen mentioned in any book. In all cases it was a *dernier ressort*, the ejection of food and the effusion of smell having preceded. W. KENNEDY.

"Finger-Print" Method.

IN my letter on the subject (NATURE, December 27, 1894, p. 199), I have introduced my assertion of the old Japanese usage of the "thumb-stamps" on legal papers, with a qualifying clause—"although at present I have no record to refer to." Continuing in my search, I have come across a passage which gives confirmation to the statement. It is in the *Fûzoku Gwahô*, No. 50, p. 6, Tōkyō, February 10, 1893, where the details of the bastinado inflicted on criminals during the *ancien régime* are given, and reads as follows:—"When the criminals' guilt was ascertained, and they signed with '*thumb-stamps*' on papers in the Court, they were sent to prison with the magistrate's words, 'Sentence shall follow,' which they used to understand as the signal of the approach of the day of punishment."

December 31, 1894. KUMAGUSU MINAKATA.

A White Rainbow.

THE white rainbow is so rare as to deserve noting. One was visible at Westnewton, Aspatria, for more than half an hour on Saturday, January 5. The band was much broader than in the ordinary bow, and the arc was formed in the upper intermediate cloud drift. This drift consisted of a light pallium of irregular cirro cumulus. It is important to observe that cumulus was forming, from above, at the time; *i.e.* the cirro cumulus was melting and descending into ordinary cumulus. A patch of this cumulus formed (under observation) and crossed beneath the bow. It then became coterminous with the western section of the arc, which blended with the cloud, and was of similar tint. Hard, dry frost continued and lasted till January 13. Barometer steady at time.

SAMUEL BARBER.

Westnewton, Aspatria, January 9.

P.S.—Connote with the above the condition of the weather on the Continent; also violent thunderstorms on following day in Cornwall; also snowstorms in Cumberland and Scotland within few days.—S. B.

AMERICAN TOPOGRAPHY.[1]

WE have it on the authority of Prof. Gannett that, at the present rate of progress, the series of topographical maps of the United States, which was commenced in 1882, will require no less than fifty years for completion, and that the cost of this great undertaking will not fall far short of twenty million dollars. The map is primarily intended to meet the needs of the geologists of the Survey; but it has been thought economical to make such arrangements that the resulting map may be adequate to serve all purposes for which general topographic maps are used. Its scope is limited to the representation of the larger natural features, and the artificial features which are of general or public interest, to the exclusion of those which are purely of a private character, and therefore liable to rapid changes.

In the vast area covered by the United States, there is a great diversity both of natural and cultural features, and the extent of the survey and the scales of the maps

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[1] "United States Geological Survey. A Manual of Topographic Methods." By Henry Gannett, Chief Topographer. (Washington: Government Printing Office, 1893.)

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